

From the Nambu-Goto to the σ -Model Action, Memoirs from Long Ago

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ABSTRACT. In this article I describe my own stumbling in the first string era. This was a time when most of the active people were very young, not very knowledgeable and the field was completely new. Many of us had little training for what we came to work on, and it took quite some time to accomplish the new conceptual discoveries.

Contribution to the volume "The Birth of String Theory"

1 Introduction

My generation of string theorists was very fortunate. We were there when the first ideas leading up to string theory were proposed, and we were young and inexperienced enough not to ask too deep questions. We could accept working in 26 dimensions of space-time, even when more experienced people laughed at it (and us). We were not more clever than they were, not at all, rather we got so attached to the ideas that we did not listen to good advice. The average age of the active people was probably well under thirty, and it was the last occasion where a young generation could form its scientific future. There were a number of older heroes, most notably Yoichiro Nambu, Stanley Mandelstam, Sergio Fubini and Daniele Amati. Also, the leading theoretical physicist of those days, Murray Gell-Mann, was sympathetic. His words, always carefully phrased, were listened to by all people in particle physics. This blend made the field so exciting that once hooked it was difficult to leave the field. After some years many had to leave in order to find positions, but most of them had the secret wish to return to this subject.

2 The Formative Years

I started as a graduate student in 1967. Sweden still had the old system, which meant that there were no graduate schools. You had to study on your own, and you had to work on your own. Every year the department of theoretical physics in Göteborg accepted a few graduate students, and the professors could handpick them. The first year was spent reading books and taking oral exams once we had finished studying a book. My advisor Jan Nilsson soon told me to work on phenomenology, and I got in contact with an experimental group in Stockholm, since we had no particle experimentalists in our physics department. For some long forgotten reason, I came across the paper by Dolen, Horn and Schmid [1] on finite-energy sum rules during my first year, and I gave a seminar on it. I tried to follow the subject and collected preprints but had no one to discuss it with in our group, who mostly worked on various forms of group theory.

I was in Stockholm in September 1968 when the professor of the experimental group, Gösta Ekspong, came in one day very excited showing everybody a paper in which some Italian had found a formula for pion-pion scattering with no free parameters other than a coupling constant. That was the paper by Gabriele Veneziano [2]. Ekspong came straight from the Vienna conference. Again I tried to follow the subject and to study all the new concepts that appeared but back home no one was interested. Instead I had to concentrate on my studies of proton-proton scattering to explain the “Deck peak,” which was essentially the $\Delta(1236)$ resonance, and to use OPE, which everybody knows means One-Pion-Exchange. I wrote a few papers on it and got my licentiate degree, which is a lower doctor’s degree. After that I felt freer to study more theoretical subjects and my advisor encouraged me to do so and, mainly together with some

visitors in the department, I wrote papers on current algebra and eventually on Dual Models. These were very simple calculations with long forgotten results, but it was a training ground and I learned a lot. I was encouraged to apply to CERN and, to my surprise and enormous happiness, I was accepted and offered a fellowship from June 1, 1971, a few months earlier than the rest of the newcomers.

3 The CERN Years

Life is often formed by accidental events. I came to CERN in the beginning of the summer and met only people who were already established in Geneva and at CERN. One month after me David Olive came to take up a staff position leaving his job in Cambridge. We became good friends almost immediately. We were the only two that summer at CERN's Theory Division facing the problems that all newcomers have when they come to CERN for a longer stay. In the beginning we were also without our families, so we spent a lot of time together, not so much discussing physics as discussing practical matters. David was, of course, already famous having been one of the leaders of the Cambridge school in the analytic S-matrix. He was one of the old-timers (he was over 30!) who had moved into Dual Models, seeing it as a realization of an S-matrix theory. Also during the summer I met John Schwarz, forming a lifelong friendship; he was visiting CERN for some weeks. I even taught him how to drive a car with a regular gear-box. He had rented a car for a trip with his mother and had never before driven such a car. (Many years later Chen Ning Yang asked me if John had been my advisor and I almost said, “no, but I was his driving instructor.”)

When I came to CERN I was still very hesitant about what problems to work on. I spent the summer working with some short-time visitors on “inclusive cross sections,” but I also followed all the seminars on Dual Models. Two more lucky events happened when the new crowd arrived at the end of the summer. One was that I got a new officemate, Joël Scherk, whom I came to share the office with for almost two years. Joël had already made a name for himself with his work in Princeton with André Neveu using the Jacobi imaginary transformation to isolate the divergence in the one-loop graphs and also with their subsequent work with John Schwarz and David Gross [3]. When he came he had just invented the “zero-slope limit” [4]. One of the first days after he arrived he gave a seminar about it in the small seminar room, and I still remember Bruno Zumino’s excitement afterwards. I overheard him say to Mary K. Gaillard that this must have something to do with quantum field theory. (This was the starting point for Bruno’s interest in dual models and led to his and Julius Wess’s discovery of four-dimensional supersymmetry a few years later. Bruno who had an office near ours used to come to us and borrow all the important papers on Dual Models.) Joël looked like a genius, talked like a genius and indeed was a genius. He had long hair and some fantastic clothes. He spoke very softly and was always very nice to talk to. We forged a deep friendship that was very close all the time up to his too early death in 1980 at only 33 years of

age. We always had a nice discussion when he arrived in the morning, usually about physics but often about life or Chinese history, which he was studying on the bus to and from Geneva.

The other event that happened was that Holger Bech Nielsen reluctantly came to CERN. He could stay for a year or more if he so wanted. He came with his mother. When she left he stayed on in a hotel for nine months until he went home. Holger was regarded as *the* genius in the field. In his suit, which after some time had seen better days, and his bow-tie he looked different. He could concentrate completely on a problem; they could have dropped an atomic bomb in the next room without disturbing him in the slightest. He had the most remarkable ideas, which nobody else had ever thought about. He spent all his time at CERN, eating all the meals there and went back to town with the last bus. I am sure that he sometimes missed it and then he walked. I had met Holger before, and he became my entrance ticket to the Dual Model Community at CERN. We started to work together – mostly on his ideas. Our main aim was to find new more realistic dual models. I did learn a lot but our progress was not great. At some stage we used duality to get sum rules for meson masses assuming a string with quarks at the ends. They worked pretty well but were very sensitive to details, since we used partition functions that involved sums of exponentials. After a year at CERN I had learned a lot but not written any really good papers, and then Holger left.

It had, of course, been a very successful year at CERN in Dual Models with the no-ghost theorem proved by Peter Goddard and Charles Thorn [5], (Charles had the third desk in our office for the year he spent at CERN) and then their work with Claudio Rebbi and Jeffrey Goldstone on the string [6]. There were lots of seminars and lots of discussions. There were several collaborations going on, but by the end of the summer David Olive and I found ourselves a bit left out. We started to discuss and David then had the brilliant idea of trying something really hard. (David always wants to study deep and hard problems.) He suggested that we should try to compute the one-loop graphs correctly. After the marvelous paper by Lovelace [7] in 1971 on one-loop graphs, where he saw that by dividing out two powers of the partition function and taking the space-time dimension to be 26, the twisted loop contains a series of poles instead of unphysical branch-cuts, it was assumed that this should be the rule for all one-loop graphs, but it was not proven. Nobody at that time had a clear idea how to prove it.

This was just a year after the gauge theory revolution and my generation and the S-matrix one, which was slightly older, knew very little about gauge theories. We had learned QED, but our knowledge of non-abelian gauge theories was rudimentary. The wonderful talk by Feynman in Poland in 1963 [8] and the subsequent work by Faddeev and Popov [9] on the construction of a one-loop graphs in non-abelian gauge theories were not known. The longer version of the Russian paper was in fact only written in Russian. After the gauge theory revolution it was quickly translated into English by David Gordon. David Olive and I started to study that paper in detail as well as some marvelous lecture notes by Abdus Salam, who as usual had immediately grasped the importance

of the subject. At the same time we studied Gerhard 't Hooft's papers.

A funny story is that the Faddeev–Popov paper discusses two different gauge choices and concludes that they give the same one-loop graph but not necessarily the same higher loop graphs. We were rather mystified by that argument and asked Gerhard, who had just arrived as a Fellow, for a discussion. Gerhard then said very emphatically that in his paper he had shown that it worked to all orders. No more discussion. Our problem was that we had no Lagrangian formulation of Dual Models – only tree diagrams. We then devised a method in non-abelian gauge field theory of starting with a naive one-loop graph and then deriving corrections by implementing gauge invariance at the one-loop level. In this way we found the ghost contributions correctly, and we then tried this method on Dual Model loops. We worked heroically with enormous algebras, but we could not finish it. (Many years later we realized that we had used the wrong Virasoro generators. We had not thought of also introducing ghosts at the two-dimensional string level.) We then went back to square one and were told about Feynman's lecture in Poland by Josef Honerkamp, who was a very knowledgeable field theorist. Feynman had been interested in quantum gravity, but Murray Gell-Mann had suggested to him that he study non-abelian gauge theories first as a warm-up exercise. He talked about this in the conference. In the discussion session after the talk he was asked by Bryce DeWitt how to compute one-loop graphs. Feynman then described in words a method where you sew together tree-diagrams using a projection operator onto the physical states. He said that one could interpret the result as if two scalar ghost fields propagated through the loop. This became the starting point for us.

Since there was no literature on this method except Feynman's words we started by redoing it in field theory. There the projection operator was known and easy to construct. However, for Dual Models we had to construct such an operator. We knew that the physical states in the critical dimension were given by the Del Giudice–Di Vecchia–Fubini operators $A_n^i(k)$ and their conjugates, where $n = 1, \dots, \infty$ and $i = 1, \dots, d-2$. The vector k is lightlike. The projection operator could then be formally written as

$$\mathcal{T}(k) = \oint \frac{dy}{2\pi i} y^{\mathcal{L}_0 - H - 1}, \quad (1)$$

where

$$\mathcal{L}_0 = \sum_{n=1}^{\infty} \sum_{i=1}^{d-2} {A_n^i}^\dagger(k) A_n^i(k), \quad (2)$$

and

$$H = \sum_{n=1}^{\infty} \sum_{\mu=1}^d \alpha_n^{\mu\dagger} \alpha_{n\mu}, \quad (3)$$

with α the ordinary harmonic oscillators of the bosonic string, which create all the excited-string states including the negative-norm ones.

By the use of operator product expansions and the shifting of integration contours, we could prove the following identity for $d = 26$

$$\mathcal{L}_0 - H = (D_0 - 1)(L_0 - 1) + \sum_{n=1}^{\infty} (D_{-n} L_n + L_{-n} D_n), \quad (4)$$

with L_n the ordinary Virasoro generators and D_n a new set of operators. It is then easy to see that the projection operator is equal to one on a physical state and zero otherwise giving us our own proof of the no-ghost theorem [10].

With this projection operator we could set up and prove Feynman's tree theorem in detail and then apply the same technique to the one-loop Dual Model loops. After a lengthy calculation we could prove that it did divide out two powers of the partition function in the measure as Lovelace had anticipated [11].

We did not dare to send the paper to Feynman, but some months later we got a letter from him that I still have on my office wall. He was extremely nice to us and thanked us for writing up his theorem "with clarity and simplicity." John Schwarz, who had moved to Caltech at that time, had shown him the paper and he had read it carefully.

The construction of the projection operator was like opening the tap. We quickly redid the same calculations for the Neveu-Schwarz model, and Corrigan and Goddard computed the projection operator for the Ramond model. We also did the calculations for the closed-string (or "Pomeron") sector at that time and proved that the Reggeon-Pomeron vertex respected unitarity [12]. We did this by commuting the projection operator from the Reggeon (open-string) sector through the vertex to the Pomeron (closed-string) vertex showing that the correct projection operator appeared on that side. This work we did with Joël Scherk, who now was brought into our collaboration. We also did the same calculation for the fermion-emission vertex showing that indeed the Ramond and the Neveu-Schwarz sectors were unitarily related. For this work Claudio Rebbi also joined in [13].

All through this period I still had contact with Holger Bech. In the spring of 1973 he came down to CERN for a week. He brought with him a mathematical way to compute $-1/12$ as the regularized sum of all positive integers. We thought hard how to connect this to strings and realized quickly that by summing up all the zero-point fluctuations of all the harmonic modes of the bosonic string, we got just the sum of all integers. We invented a physical way to regularize by renormalizing the velocity of light which is a parameter of the Nambu-Gotō string. In this way we got an alternative proof of $D = 26$ (my third by then) [14]. It was obvious to me that the Ramond fermions must be massless since the zero-point fluctuations canceled between the bosonic and the fermionic ones. David, even though he is a born gentleman, persuaded me not to publish it, since we were in the midst of our fermion calculations and the common belief at the time was that the fermion had a non-zero mass. I wrote it up in my thesis later that year.

At this time in the beginning of the summer of 1973 my time was up at CERN. So I moved home to Sweden becoming very depressed. Only when I had

left did I realize what a fantastic place CERN had been for the development of string theory. This was due to Daniele Amati who tirelessly defended us, as we understood much later, and who gave so much of his time and of himself to us Fellows. At home I tried to communicate with David Olive, but this was before the internet. Fortunately, it was also before the demise of the postal services, so we could get letters through in less than 24 hours. The final paper in this stage of our collaboration was the construction of the four-fermion amplitude that David and Joël constructed during that summer [15].

Back home I also had to finish my thesis, which came to consist of fourteen papers and an introduction of one hundred pages. It was the longest thesis in the history of the department. After defending it I resumed the collaboration with David Olive, and we worked hard to understand the fermions in Dual Models. In the summer of 1974 John Schwarz organized a workshop in Aspen and most people who had been involved in the developments were there except for Pierre Ramond, Charles Thorn and Holger Bech. Charles was already working on the MIT-bag and Pierre was busy with the birth of his second daughter. Holger's interest in string theory had started to fade, and he was full of other interesting ideas. When I came to Aspen Bunji Sakita told me that Professor Nambu wanted to talk to me. I was quite excited and thought he would comment on all the work that David Olive and I had done. No, he congratulated me instead on the paper with Holger about zero-point fluctuations. This was very flattering, because I consider Yoichiro Nambu to be one of the greatest scientists of all time.

4 Collaborations at Nordita

I was lucky in one sense compared to my friends and collaborators. There was no pressure on me to change to a more fashionable subject. My situation in Sweden was stable but not very stimulating. I had had a research position with the research council even before going to CERN and I took it up again when I got home. It was renewed every year; on the other hand, there were no more permanent jobs to apply for.

When I came home after the 1974 summer at Aspen, Paolo Di Vecchia arrived to Nordita in Copenhagen as an assistant professor. That was to become very important for me. Nordita is a Nordic institute and its mission is to promote theoretical physics in the Nordic countries. I could travel to Copenhagen more or less whenever I wanted as long as Paolo agreed, and he was also so generous that I could stay with him when I came there. In the beginning I still worked with Holger Bech finishing up some old ideas, but Paolo and I discussed more and more. Paolo wanted to construct a fermionic string by starting with x^μ and a space-time spinor θ^α . He constructed the obvious invariant and was on his way to construct the Superstring. I wanted to have a string action for the Ramond–Neveu–Schwarz Model, so I was skeptical. In the ski season of 1975 I visited CERN and had a long discussion with Bruno Zumino. Exactly at the same time Bruno and I said that we should have a two-dimensional spinor

instead and try to have reparametrization invariance on the world-sheet. Several people, including Gervais and Sakita and Mandelstam, had worked earlier with such spinors, but they had not constructed an action from which the full constraint algebra follows. This seemed like a good problem to work on, and I convinced Paolo to join me. We had no understanding of Grassmann algebras and had to start from scratch. Fortunately, there was the wonderful book by Berezin [16]. We, of course, wanted to extend the Nambu-Gotō string to two-dimensional superspace but found no way of doing it. We wanted to construct a square-root of something but always stumbled on the strange properties of the Grassmann variables. We also knew so little about general relativity and the difficulty of including fermions, since neither of us had studied any courses in General Relativity. After a while we realized that we first ought to solve the corresponding point-particle problem, but we ran into the same problem there.

In the summer of 1975 there was a workshop in Durham that David Fairlie organized. That was the first time that I met Pierre Ramond. I was already at the college where we stayed when David came in with a new person, very French-looking. David introduced him to me and Pierre said “Oh, my God, Lars Brink”. That was a perfect beginning to a life-long friendship. At the meeting I realized that the superfield formalism that we had developed was ideal for a super-operator formalism and later that year I worked it out with my student and formulated the superconformal formalism that later was reinvented in the second string era [17]. At the meeting there was a crowd of Italians, all good friends of Paolo, and we started to discuss the superfield representation we had so far for the RNS string. We could write a free action for the superfield and then implement the Noether current as the constraints. It worked but was hand-waving. Soon we realized that we could extend the supersymmetry and we all met at CERN in September to work out the $SO(2)$ case. In this way we got an extended super-Virasoro algebra, the first one. The key was that the Noether current also involved a Kac-Moody $U(1)$ current. We went on and constructed an infinite sequence of extensions [18] but only the $N = 2$ and $N = 4$ cases were interesting. By using that $SO(4) = SU(2) \times SU(2)$ we could construct the $SU(2)$ algebra. This one and the $SO(2)$ one were the only ones with canonical operators. For the rest of that year and the beginning of the next one we were busy formulating models for these cases. After our first paper on the new super-Virasoro algebras I got a letter from John Schwarz who had immediately understood our formalism and also constructed the $SO(2)$ model. We invited him to join us on that paper, and then the authorship consisted of eleven Italians, one American and one Swede [19]. Shortly after John wrote me that Caltech had some money free for the coming academic year and wondered if I was interested. I was already negotiating with CERN to be a Corresponding Fellow that year, so John’s offer came at a perfect time and I accepted readily.

In the spring of 1976 we wanted to go back to the problem of finding a reparametrization invariant action. We slowly got to understand the supergravity action [20] that had been constructed in the beginning of that year. Sergio Ferrara, who had been a member of the our huge collaboration, had moved to Paris for a year, and we should have connected quicker to what they had done.

I was very busy that spring and summer getting another child and moving into a new house. Only in the summer did we manage to meet and work out the particle action. In fact, we met at CERN for a few days to finish it and were joined by Paul Howe who was a postdoc at Nordita and by Stanley Deser and Bruno Zumino [21]. The key point, which we had missed before, was the use of vierbeins (or, rather, as Murray Gell-Mann named the ones in general dimensions, “vielbeins”). Once we understood it, it was rather straightforward to solve the particle problem and, as a result, we got a world-line action that leads to the Dirac equation.

We realized that we now could construct the string action, too, but it took some time before we could meet to finalize it. One problem for me was that I was planning the trip to Caltech and I had to make lots of preparations for that. Finally, we met for a week and worked like mad to construct the action and to prove all its local symmetries and then to show that it leads to the constraints of the RNS model [22]. Again, we were inexperienced with the use of spinors and knew nothing about Fierz rearrangements, so we had to do it the hard way. Anyhow, I went home to get the manuscript typed by our secretary, and then I sent it out. More or less with return mail I received the paper by Deser and Zumino [23] that contains the same action (of course.)

The following week I left for Caltech. The first week there John and I (very jet-lagged) constructed the $N = 2$ action using the same technique [24]. It is interesting that it was this paper that Sasha Polyakov read when he came to Caltech the next year and learned about these actions. He had in fact been in Copenhagen for an extended period when we constructed the action, but he was so intensely engaged in his magnificent work on instantons and confinement that he had missed our work. Of course, few people took notice, since it was so far from the mainstream. We did meet each other then briefly, though, and it was also a start of a life-long friendship.

5 Leaving Strings for a While

After our first paper together at Caltech, John and I felt that we must work on more modern stuff. We wanted to use all our insight from string theory on supersymmetric field theories. Naturally, we started with the ten-dimensional super Yang–Mills theory and by compactifying it to various dimensions we found other maximally supersymmetric gauge theories including the $N = 4$ theory in $D = 4$. We got in touch with Joël in Paris, who had been doing the same thing, and we wrote it up together [25]. We did feel that this was an important model but little did we know that it should be one of the cornerstones of modern theory. (Some five years later I returned to it with Bengt Nilsson and Olof Lindgren when we finally found a way to prove perturbative finiteness [26].) We wanted though to reformulate supergravity, and we teamed up with Pierre Ramond and Murray Gell-Mann and worked hard on supergravity in superspace. I was very insistent on using superspace, since I had fallen in love with it in our studies of supersymmetric strings. We worked on this for quite some time and

reconstructed the first supergravities this way [27]. Eventually in 1979 Paul Howe and I managed to construct the $N = 8$ supergravity in superspace [28]. When he and Ulf Lindström [29] using our formalism the year after showed that there were possible counterterms in that theory I went back to string theory and joined up with John Schwarz and Michael Green. My attachment to strings was a love for life.

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